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tional institutions and opportunities for a career than to stock, and is thus evidence in favor of scientific productivity being in the main due to opportunity rather than to heredity. It is probable that if the 174 babies born in New England who became leading scientific men had been exchanged with babies born in the south, the scientific productivity of New England would not in that generation have been materially decreased, nor the scientific productivity of the south have been greatly increased. It is certain that there would not have been 174 leading scientific men from the extreme southern states and only seven from Massachusetts and Connecticut. If the stock of the southern states remains undiluted, it may, as social conditions change, produce even more scientific men per thousand of its population than New England has hitherto produced. Japan had no scientific men a generation ago and China has none now, but it may be that in a few years their contributions to science will rival ours.

The second point discussed by Dr. Woods is my qualified inference that the fact that those regions which have produced more scientific men have not produced men of higher average performance is against the theory that scientific productivity is mainly due to heredity. Dr. Woods says that this would doubtless be a very strong argument if it should be substantiated by further statistics. His discussion of my statistics does not seem to alter the interpretation put on them. He, however, brings forward new data of interest, which show that the scientific men produced by Massachusetts are slightly above the average and that Massachusetts has produced far more than its share of men of unusual eminence. These facts do not, however, affect my argument. It would be expected that the educational advantages and opportunities for research in Massachusetts would give its scientific men a higher average standing than those elsewhere, even though their native ability were the same. It is surprising that this does not. Some cockerells can be trained better than others, but there are innumerable cockerells that might be trained and are not.

not show at all in the 1,000 leading men of science and but slightly in the 4,131 included in the "Biographical Directory." In the case of men of exceptional genius, I agree with Dr. Woods that they can not be regarded as the product of their environment. But it may interpose a veto on their performances. There may be "mute inglorious" Emersons in southern churchyards. Lincoln was as great a writer as Emerson; but it is in a way a chance that he made his Gettysburg speech. It is likely, but not proved, that one region of this country or one of its racial stocks has more potential men of genius than another.

While views such as those of Dr. Galton when he says "The impression that all this evidence leaves on the mind is one of some wonder whether nurture can do anything at all" or of Professor Pearson when he says "We inherit our parents' tempers, our parents' conscientiousness, shyness and ability, even [to the same extent] as we inherit their stature, forearm and span," seem to be extreme, I hold, as stated in the paper quoted by Dr. Woods, that "kinds of character and degrees of ability are mainly innate." But I believe also that there is in this country a vast amount of the character and ability required for scientific productivity which is not used for this purpose, and that the quantity, though not the quality, of our scientific work could be increased to almost any extent. What a man can do is prescribed by heredity; what he does is determined by circumstance.

J. McKEEN CATTELL

GENERA WITHOUT SPECIES

THE views on genera without species held by Dr. J. A. Allen, as expressed in *SCIENCE*, June 11, 1909, may possibly be shared by a few entomologists interested in restricted groups and by many students of higher forms of life, such as birds and animals. It is not remarkable that an ornithologist or mammalogist, whose entire number of subjects scarcely equals that of the species of a single family of some orders of insects, should hold that personal judgment should enter into the solving of this important problem. It is the man

who is concerned with the hundreds of thousands of names rather than he who deals with the thousands that sees most clearly the hopelessness of gaining stability by methods where personal opinion is given full sway. Dr. Allen attributes the remarkable unanimity of opinion of those opposed to his views to inexperience or ignorance of the subject. If the worthy doctor himself was more experienced in the fields of entomology or botany, where the forms are countless as compared with animals or birds, he might be less positive in his position on this question. It may not be absurd to state that an ornithological genus based on an unnamed woodpecker with three toes can not be mistaken because but one such bird was known. But would it seem so plausible to state that a genus of insects based on an unnamed specimen of parasitic hymenoptera, or a minute fly, with a certain vein of the wing forked before the middle was unmistakable because but one such species was known while many thousands of such little creatures are flying undescribed about us?

It is true that this question is not definitely covered by the International Code, but certain statements do have a bearing on the subject. On page 11 of the code the generic and specific name is likened to the family and individual names of persons. Now who can conceive of a family of Smiths without a John or a Jane in it? Would it not seem silly to have a name Johnson before any one was born to bear it? Getting back to genera, what is a genus? "An aggregation of one or more species" would seem to be a good definition. If such a definition was accepted it would certainly invalidate the genus without species, so I presume Dr. Allen has another definition. Not knowing what it is, I can not discuss it. The code does not define the genus. However, it is now quite universally agreed that a genus should have a type designated. Article 30 of the code, paragraph 2, says: ". . . nor can a species be selected as type which was not originally included in the genus. . . ." This being true, how can we get a type for a genus where there were no species originally in-

cluded? In the amendments to the code, published in *SCIENCE* for October 8, 1907, is the following: "The commission is unanimously of the opinion that a *name*, in the sense of the code, refers to the designation by which the actual objects are known." Now a genus without a species has no object; it is a name applied to a conception, not to an object and can therefore have no place in systematic nomenclature.¹

No one, I think, claims infallibility for the international code; but it is certainly not to the best interest of nomenclatorial stability to knowingly violate its recommendations. An able board of chosen nomenclaturists has passed on and sanctioned these rules and formulated them into an accessible code, and it should be incumbent upon systematists to comply with them so far as possible. There are enough questions not covered by the code to furnish constant contention without bringing up problems that are capable of being disposed of under the rules already formulated. That which is best in one group may not be the best for another, but for the sake of uniformity and in the hope of future stability let us accept the dictum of the International Zoological Congress and follow the code.

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A NOTE ON UROPHLYCTIS ALFALFÆ (V. LAGERH.)
P. MAGN. IN CALIFORNIA

A CROWN gall of alfalfa (*Medicago sativa*) which occurs in Europe, but which, so far as known by the writer, has not before been noted in this country, has recently come to our attention in California.

The disease was first observed in Ecuador in 1892 by Lagerheim, who placed the parasitic fungus causing it in the genus *Cladochytrium*. In 1902 it was found in Alsace, Germany, by Magnus, who transferred the organism to the genus *Urophlyctis*. It has since been observed in other localities on the continent, where it has done considerable damage.

The galls are usually very numerous at the

¹ *Nomen nudum* does not seem inappropriate in this connection.